called perpetual motion. Modern science seems to show that it is equally vain to seek for anything that is perpetually and absolutely at rest.

I have alluded to the kinetic theory of gases because we know more of the constitution of that form of matter than we do of any other, but having regard to the progress of science to which I have referred, is it too much to hope that some of you will live to see a second Newton, who will give you a second Principia, which shall clear away the difficulties which surround the constitution of matter whether ponderable or imponderable?

One word more, bring enthusiasm to your studies; without it the best instruction (this you will have) and the best apparatus will do nothing for you. Make your work the first aim, and do not let athletics, or anything else, take precedence of it. Here, again, I cannot help thinking that the Germans get a little the better of us. With them work is absolutely in the forefront; I am not at all sure that it is so with the average young Englishman of to-day. No one appreciates the value of athletics, when kept in their proper place, more keenly than I do. But against the substitution of athletics for the more serious objects of life, I should like to enter my strongest protest, and it will be a sorry day for England if such a change ever takes place.

Lastly, I would say to you, while giving the acquiring of knowledge that may assist your own business or profession the first place, not be too utilitarian, do not narrow the search for knowledge down to a search for utilitarian knowledge, for knowledge that you think will pay. I remember a strong protest of De Morgan's against the number of men who take their station in the business of life without ever having known real mental exertion; he put it that knowledge which ought to open the mind was decided on solely by its fitness to manure the money tree.

Therefore, above all things, pursue knowledge. It is that pursuit which will stand by you to the end as at once the greatest and the most enduring of pleasures. Friends may die; the most tender attachments must be severed; advancing years will very soon debar you from any serious pursuit of athletics; the acquisition of wealth will take away from you the pleasure of "making a position," which is probably the keenest, and surely the most legitimate, incentive of middle life; but the pleasure of acquiring knowledge will console you to the last, so long as you have strength to open a book, or to hold a test-tube. Cry after knowledge; seek for her as silver; and search for her as for hidden treasure.

THE BRITISH ASSOCIATION.

SECTION H.

ANTHROPOLOGY.

OPENING ADDRESS BY C. H. READ, PRESIDENT OF THE SECTION.

THE difficulties that beset the President of this Section in preparing an address are chiefly such as arise from the great breadth of our subject. It is thought by some, on the one hand, to comprehend every phase of human activity, so that if a communication does not fall within the scope of any other of the Sections into which the British Association is divided, it must of necessity belong to that of anthropology. On the other hand, there are many men, wanting neither in intelligence nor education, who seem incapable of grasping its general extent, but, mistaking a part for the whole, are fully content with the conclusions that naturally result from such a parochial method of reasoning. The Oxford don who stated, a year or two ago, his belief that anthropology rested on a foundation of romance can only have arrived at this opinion by some such inadequate process, and the conclusion necessarily fails to carry conviction. The statement was, however, singularly ill-advised, for anthropology gives way to no other branch of science in its reliance upon facts for its existence and its conclusions. Had the reproach been that the facts were often of a dry and repellent character we might have pleaded extenuating circumstances, but I fear it must have been admitted that there was some justice in the complaint, though we could fairly point to instances where master minds have made even the dry bones of anthropology live, and that without trenching upon the domain of romance.

It is not, however, my purpose to-day to enter upon a general defence of anthropology as a branch of science. It has taken far too firm a hold upon the popular mind to need any such help. I intend rather to treat of one or two special subjects with which I am in daily relation, in order to see whether some practical means cannot be found to bring about a state of things more satisfactory than that at present existing.

The first of these branches is that of the prehistoric antiquities of our own country. It will not be denied that there can be no more legitimate subject of study than the remains of the inhabitants of our islands from the earliest appearance of man up to the time when written history comes to the aid of the archæologist. There is no civilised nation which has not devoted some part of its energies to such studies, and many of them under far less favourable circumstances than ours. The chiefest of our advantages is to be found in the small extent of the area to be explored—an area ridiculously small when compared with that of most of the continental nations, or with the resources at our command for its exploration. The natural attractions of our islands, moreover, have also had a great influence on our continental neighbours, so that their incursions have not been few, and no small number of them decided to remain in a country where the necessaries of life were obtainable under such agreeable conditions. The effect of these incursions, so far as our present subject is concerned, is that there is to be found in the British Islands a greater variety of prehistoric and later remains than is seen in most European countries, a fact which should add considerably to the interest of their exploration. At the same time also it must be borne in mind that it is by such researches alone that we can arrive at any true understanding of the conditions of life, the habits and religious beliefs, or the physical characters of the varied races who inhabited Britain in early times.

It may seem unnecessary to urge, in face of these facts, that all such memorials of the past should be, in the first place, preserved; and, in the second, that any examination of them should be undertaken only by properly qualified persons. Unfortunately, however, it has never been more necessary than it is at the present time to insist upon both points, and the fact that these prehistoric remains are scattered impartially over the whole country, with the exception, perhaps, of the sites of ancient forests, makes it almost impossible to devise any special measures for their preservation. An additional difficulty is to be found in the fact that many ancient remains, such as the barrows of the early Bronze Age, are altogether unrecognised as such, and in the process of cultivation have been ploughed down almost to the level of the surrounding surface, until at last the plough scatters the bones and other relics unnoted over the field, and one more document is gone that might have served in the task of reconstructing the history of early man in Britain.

Such accidental and casual destruction is, however, probably unavoidable, and, being so, it is scarcely profitable to dwell upon it. We can, perhaps, with more advantage protest against wilful destruction, whether it be wanton mischief or misplaced archæological zeal. An enlightened public opinion is our only protection against the first of these, and will avail against the second also, but we are surely entitled to look for more active measures in preventing the destruction of archæological monuments in the name of archæology itself. It is a far more common occurrence than is generally realised for a tumulus to be opened by persons totally unqualified for the task either by experience or reading. An account may then be printed in the local journal or newspaper. When such accounts do appear it is often painfully obvious that an accidental and later burial has been mistaken for the principal interment, while the latter has been altogether overlooked, and no useful record has been kept of the relative positions of the various objects found. The loss that science has suffered by this indiscriminate and ill-judged exploration is difficult to estimate, for it should be borne in mind that an ancient burial, once explored, is destroyed for future searchers—no second examination can produce results of any value, though individual objects overlooked by chance may repay the energy of the later comers. So much varied knowledge is, in fact, required for the proper elucidation of the ordinary contents of a British barrow that it is almost impossible for any single person to perform the task unaided. A wide experience in physical anthropology must be combined with an acquaintance fully as wide with the ordinary conditions of such interments and the nature, material, and relative positions of the accompanying relics, all of which must be brought to bear, with discriminating judgment, on the facts laid bare by the digger's spade. Added to this, the greatest precaution is needed that nothing of value be overlooked. In

some soils, such as a stiff clay, it is almost impossible to guard against such a casualty, especially when the barrow is of large size and vast masses of earth have to be moved. The amount of profitable care that may be bestowed on scientific work of this character can nowhere be better seen, I am glad to say, than in our own country, in the handsome volumes produced by General Pitt-Rivers as a record of his investigations in the history of the early inhabitants of Dorsetshire. The memoirs contained in them are unsurpassed for scientific thoroughness, and they will probably long stand as the model of what archæological investigation should be. It is very seldom, however, that circumstances conspire so favourably towards a desired end as in the case of General Pitt-Rivers, where a scientific training is joined to the love of research, and finally ample means give full scope for its practice under entirely favourable conditions. While it is, perhaps, too much to expect that all explorations of this character should be carried through with the same minute attention to detail that characterises General Pitt-Rivers's diggings, yet his memoirs should be thoroughly studied before any work of the same kind is entered upon, and should be kept before the mind as the ideal to be attained. It is not too much to say that a diligent study of the works of the two foremost explorers of prehistoric remains in this country—Canon Greenwell and General Pitt-Rivers—will of itself suffice to qualify any intelligent antiquary to conduct the exploration of any like remains. At the same time, it must not be forgotten that exploration is one thing and a useful record of it is another, and here the explorer would do well to invite the co-operation of specialists if he would get the full value out of his work, and there is generally little difficulty in getting such help.

I have ventured to point out, in moderate terms, the dangers to which a large number of our prehistoric sites are liable, and to state under what conditions they should be investigated. It is not unreasonable to expect, if the danger is so obvious, that a remedy should be forthcoming to meet it. In most of the continental States it would be easy to institute a scheme of State control by which such sites would vest in the Government to just such an extent as would be necessary to prevent their being destroyed, and such a scheme might be cheerfully accepted and applied with success in any country but our own. Here, however, we are so accustomed to rely upon individual influence and exertion in matters of this kind, that an appeal to the Government is scarcely thought of; while, on the other hand, the rights of property are fortunately so safeguarded by our tradition and law that nothing but a futile Act of Parliament would have the least chance of passing. Moreover, experience teaches us that it is not to State control that we must look. The Ancient Monuments Bill, which was intended to protect a special class of monuments, and was framed with a full regard to the rights of owners, still stands in the Statute Book, but for years past it has had no effective value whatever. That being so, we must look to private organisations, and preferably to those already in existence, for some effectual moral influence and control, and, in my judgment, the appeal could best be made to the local scientific societies. Many of these are very active in their operations, and could well bear an addition to their labours; others, less active, might become more energetic if they had a definite programme. The plan I would propose is this:—Each society should record on the large scale Ordnance map every tumulus or earthwork within the county, and at the same time keep a register of the sites with numbers referring to the map, and in this register should be noted the names of the owner and tenant of the property, as well as any details which would be of use in exploring the tumuli. I am well aware that a survey of this kind has been begun by the Society of Antiquaries of London, and is still in progress; but this is of a far more comprehensive character, and is, moreover, primarily intended for publication. The more limited survey I now advocate would in no way interfere with it, but, on the contrary, would provide material for the other larger scheme. Once the local society is in possession of the necessary information just referred to, it would be the duty of its executive to exercise a beneficent control over any operations affecting the tumuli, and it may safely be said that such control could in no way be brought to bear so easily and effectively as through a local society.

Some of the arguments in favour of some such protection for our unconsidered ancient monuments have been already briefly stated, and, in conclusion, I would only urge this in their favour, that while the more beautiful monuments of later and historic times are but little likely to want defenders, the less attractive early remains are apt to disappear not so much from want of appreciation as from want of knowledge, and I would repeat that it is from them alone that we can reconstitute the life-story of those who lived in what we may, with truth, call our dark ages.

I will now ask you to turn your attention to another matter in which it seems to me that this country has opportunities of an unusually favourable kind. I refer to the collection of anthropological material from races which still remain in a fairly primitive state. It is somewhat trite to allude to the extent of our Empire and the vast number of races either subject to our rule or who look to us for guidance and protection. The number may be variously computed according to the bias, philological or physical, of the observer, but it will not be contested that our opportunities are without precedent in history, nor that they greatly exceed those of any existing nation. That being so, it may not be useless to see how far these opportunities are utilised. While it will not be denied that the Indian Government and the Governments of some of our Colonies have done excellent work in the direction of anthropological research and publication, and that exhaustive reports from our Colonial officials are frequently received and afterwards entombed in parliamentary papers, yet it is equally clear that work of this kind is not a part of our administrative system, but rather the protest of the intelligent official mind against the monotony of routine. The material, the opportunity, as well as the intelligence and will to use both, are already in existence, and all that is now wanted is that the last should be encouraged, and the work be done on a systematic plan, and, as far as may be, focussed on some centre where it may be available for present and future use. It was for this end that I ventured to bring before the British Association at the Liverpool meeting a scheme for the establishment of a central Bureau of Ethnology for Greater Britain. Frequent appeals had been made to me by officials of all kinds in distant parts of the Empire to tell them what kind of research work they could most usefully undertake, and it seemed a pity not to reduce so much energy and good will into a system. Hence the Bureau of Ethnology. The Council of the Association, on the recommendation of the Committee, invited the Trustees of the British Museum to undertake the working of the Bureau; this they have accepted, with the result that if the Treasury will grant the small yearly outlay it will be under my own supervision. If I had foreseen this ending I might have hesitated before starting a hare the chasing of which will be no sinecure.

It was considered necessary, before attempting to begin the work of the Bureau by communicating with commissioners and other officials in the various Colonies and Protectorates, to lay the matter before Lord Salisbury and to invite his approval of the scheme. The whole correspondence will appear in the Report of the present meeting, but I may be pardoned for quoting one paragraph of the circular letter from the Foreign Office to the several African Protectorates. It is as follows: "Lord Salisbury is of opinion that Her Majesty's officers should be encouraged to furnish any information desired by the Bureau, so far as their duties will allow of their doing so, and I am to request you to inform the officers under your administration accordingly." When it is remembered that this is strictly official phraseology, its tenor may be considered entirely satisfactory, and there can be little doubt that other departments of the Government will recognise the utility of the Bureau in the same liberal spirit. Thus we shall have within a short time an organisation which will systematically gather the records of the many races which are either disappearing before the advancing white man, or, what is equally fatal from the anthropological point of view, are rapidly adopting the white man's habits and forgetting their own.

The Bureau of Ethnology, however, will only perform a part of the task that has to be done. While there is no doubt of the value of knowledge as to the religious beliefs and customs of existing savages, it is surely of equal importance that anthropological and ethnological collections should be gathered together with the same energy. The spear of the savage is, in fact, far more likely to be replaced by the rifle than is his religion to give way to ours. Thus the spear will disappear long before the religion is forgotten. It may be said that we have collection of this kind in plenty, and it is true that in the British Museum, at Oxford, Cambridge, Liverpool, and Salisbury, there are indeed

excellent collections of ethnology, while at the College of Surgeons and the Natural History Museum there are illustrations of physical anthropology in great quantity. Whatever might be the result if all these were brought together, there can be no question that no one of them meets the requirements of the time. Here also there is a want of a system that shall at once be worthy of our Empire and so devised as to serve the ends of the student. Where, if not in England, should be found the completest collections of all the races of the Empire? It must be admitted, however, not only that we have no national collection of the kind, but that other nations are ahead of us in this matter. This could be readily understood if their sources of supply were at all comparable to ours. But this is, of course, very far from being the case. The sources are ours in great part, and if we stand inactive it is not unlikely that some will be exhausted when we do come to draw upon them. It is, perhaps, better to give here a case in point rather than to rely on general statements. In the summer of last year I arranged, with the approval of the Trustees, that Mr. Dalton, one of the officers of my department, should make a tour of inspection of the ethnographical museums of Germany, with a definite object in view, but at the same time that he should make a general survey of their system and resources as compared with our own. The report which he drew up on his return was printed and has recently been communicated to the newspapers; it is therefore not necessary to allude to it now, except to quote one instance confirming my statement that it is to a great extent from our Colonies that material is being drawn. Mr. Dalton says: "On a moderate estimate the Berlin collections are six or seven times as extensive as ours. To mention a single point, the British province of Assam is represented in Berlin by a whole room and in London by a single case." But even this, forcible though it is, does not adequately represent the vast difference between the material at the disposal of the two countries. For it is the habit of the collectors for the German museums to procure duplicates or triplicates of every object, for the purposes of exchange or study. It is thus not unlikely that the whole room referred to represents only a part of the Berlin collection from the British province of Assam. In making these observations, I should be sorry if it were thought that I wish to advocate a dog-in-the-manger policy, or that I consider it either desirable or politic to place any restriction upon scientific work in our Colonial possessions, even if such restrictions were possible. I would prefer to look at the matter from an entirely different point of view. If the German people, who are admittedly practical and business-like, think it worth while, with their limited Colonies, to spend so much time and money on the establishment of a royal museum of ethnography, how much more is it our duty to establish and maintain one, and on a scale that shall bear some relation to the magnitude of our Empire. The value of such museums is by no means confined to the scientific inquirer, but they may equally be made to serve the purpose of the trader and the public at large.

How can we best obtain such a museum? That is the question that we have to answer. It is scarcely profitable to expect that the Government will be stirred to emulation by the description of the size and resources of the Museum für Völkerkunde in Berlin. In the British Museum there is at the present time only the most limited accommodation even for the collections already housed there, and I am well aware that these form a very inadequate representation of the subject.

It may be thought that the solution of this difficulty is easy. It is well known that the Government has purchased the rest of the block of land on which the British Museum stands, and it may seem that such a liberal extension as this will form should be enough for, at any rate, a generation or two, and that a little additional building would meet immediate wants, and enable the collections, now so painfully crowded, to be set out in an instructive and interesting way. I admit that if the whole of the contemplated buildings were at this moment complete, and at least double as much space given to the ethnographical collections as they occupy at present, the difficulty would be much simplified. The collections could at any rate be then displayed far more worthily and usefully. Even this, however, would hardly meet the case, even if there were a certainty of the buildings being immediately begun. Such works as these, however, can only be executed in sections during the course of each financial year. Thus, even if a Chancellor of the Exchequer could be found to fall in entirely with the views of the Trustees, it would still be an appreciable number of years before the completion of the entire range of galleries that is contemplated. For this reason alone I do not look forward to obtaining the space that is even now urgently wanted for some time. Meanwhile the natural and legitimate increase of the collections at the rate of about 1 to 2 per cent. per annum still goes on, and the original difficulty of want of room would still face us, though in a lesser degree. This estimate of the rate of increase may seem a high one; but it should not be forgotten that the science is new, and that it is only within the last few years that such collections have been made on scientific lines, instead of being governed only by the attractive character or rarity of the object. The gaps that exist in such a series as that of the British Museum, made in great part on the old lines, are relatively more numerous than would be the case in museums more recently founded. Another reason, equally cogent, for allowing far more room than is required for the mere exhibition of the objects is that, in my judgment, ethnographical collections, to be of real value, need elucidation by means of models, maps and explanatory descriptions, to a far greater extent than do works of art, which to the trained eye speak eloquently for themselves. Such helps to understanding necessitate a considerable amount of space, though the outlay is fully justified by their obvious utility, and in any general scheme of rearrangement of the national collection they should be considered an essential feature.

There is yet another factor to be considered. It has been the fashion in this country to consider remains illustrating the physical characters of man to belong to natural history, while the productions of primitive and uncultured races generally find a place on the antiquarian side. Thus the skull of a Maori will be found at the natural history branch of the British Museum, while all the productions of the Maori are three miles distant in Bloomsbury. Such an arrangement can perhaps be defended on logical grounds, but its practical working leaves much to desire, and the arguments for a fusion of the two are undoubtedly strong. For instance, the student of one branch would be unlikely to study it alone without acquiring a knowledge of the other, while the explorers to whom we look for collections usually give their attention to both classes of anthropological material. Here again, in such a case, there would be a call for still more space at Bloomsbury.

If I may be permitted to add one more to the requirements of what should be an attainable ideal, I should like to say that courses of lectures on anthropology delivered in the same building that contains the collections would form a fitting crown to such a scheme for a really Imperial museum of anthropology as I have endeavoured to sketch. There is but one chair of anthropology in this country, and admirably as that is filled by Prof. Tylor, he would himself be the first to admit that there is ample room and ample material to justify the creation of a second professorship.

It will be admitted that if my premisses are well founded the conclusion must necessarily be that we cannot look to the British Museum to furnish us eventually with the needful area and other resources for the installation of a worthy museum of anthropology. The difficulties are far too great for the Trustees to overcome, unless by the aid of such an exhibition of popular enthusiasm as I fear our branch of science cannot at present command. Failing the British Museum, which may be called the natural home of such a collection, we must look elsewhere for the necessary conditions, and I think they are to be found, although it is possible that, however favourable these conditions may seem from our point of view, difficulties may already exist or arise later.

It is not the first time that a scheme has been thought out for the establishment of a museum or kindred institution which should represent our Colonies and India. In the year 1877 the Royal Colonial Institute made a vigorous effort in this direction, and, in combination with the various chambers of commerce throughout the country, advocated the building of an "Imperial Museum for the Colonies and India" on the Thames Embankment, with the then existing India Museum as a nucleus. The arguments then brought forward were in the main commercial, but they are, if anything, more forcible now than they were twenty years ago. The competition with foreign countries has become keener on the one hand, while the bonds between the Colonies and the parent country are notoriously closer and more firm than at any previous time. No moment could thus be more opportune than the present for the foundation of a really Imperial Institution to represent our vast Colonial Empire.

The last sentence has, perhaps, given an indication of my solution of the question. The Imperial Institute at South Kensington has now been in existence for some time, and has passed through various phases. But its most enthusiastic supporters will scarcely claim for it entire success in its mission. Whatever may be the underlying causes, it must be admitted that such popular support as it possesses is scarcely founded on the performance of its functions as an Imperial Institute. It would seem, therefore, that something more is wanted—a more definite *raison d'être*—than it has at present, and this I think it will find in being converted into such a museum of anthropology as I have indicated, but, of course, as a Government institution. I am by no means an advocate of the creation of new institutions, if the old ones can adequately do their work, nor do I think that anything but ill would result from a general partition of the contents of the British Museum. The separation of the natural history from the other collections was painful, though inevitable, and no such severe operation can be performed without loss in some direction. But the removal of the ethnographical and anthropological collections from the British Museum to the galleries of the Imperial Institute would possess so many manifest advantages that the disadvantages need scarcely be considered. The Government has already taken over a portion of the building for the benefit of the University of London. The remaining portion would provide ample accommodation for the anthropological museum, as well as for the commercial side, that might properly and usefully be continued; its proximity to the natural history branch of the British Museum would render control by the Trustees easy; the Indian collections, which formed so important a feature in the scheme of 1877, are at this moment under the same roof; and finally the University of London has but to found a chair of anthropology, and the whole of the necessary conditions of success are fulfilled.

I have but little doubt that, wherever it might be placed, the creation of a distinct department of anthropology would of itself tend to the enrichment of the collections. It must be remembered that it is only since 1883, when the Christy collection was removed to the British Museum, that the ethnographical collections there can claim any kind of completeness. Until then one small room contained the few important objects of this kind that had survived from the voyages of Cook, Wallis and the other early voyagers. The public did not expect to find ethnography in the British Museum, and it is, in fact, only within the last few years that it has been generally realised that a gallery of ethnography exists there. If it were placed in such a building as the Imperial Institute, it would still remain part of the British Museum, and be under the guardianship of its Trustees; but it would obviously command more attention and support from the public than can be expected while it remains an integral part of a large institution which has as many aims as it has departments.

I began this address by stating that it would have a practical application. I trust that to others it may seem that what I have ventured to suggest is not only possible of achievement, but would also be beneficial to the branch of science that we represent. I should like to add that, as far as possible, I have tried to state the case as it would appear to one who regarded the situation from an entirely independent standpoint, and wishing only to discover the most practical solution of what must be admitted to be a difficult question. My allegiance to the British Museum, however, may well have tinged my views, unnoticed by myself. There are many other subjects that might well have formed the subject of an address at the present time. On such occasions as these, however, it is, I think, advisable to select a subject with especial reference to the needs of the time, and I know of nothing that is at the present moment more urgent in this particular direction, and in my judgment it will tend greatly towards the true advancement of science, the object we all have at heart.

SECTION I.

PHYSIOLOGY.

OPENING ADDRESS BY J. N. LANGLEY, F.R.S., PRESIDENT OF THE SECTION.

ONE might suppose that physiology, dealing as it does for the most part with structures—such as nerves, and muscles, and glands—which every one has and has heard of, would be eminently a science the newer aspects of which every one could readily understand. And in this supposition one would be encouraged by the frequency of the references in English literature to some part of our inner mechanism. More than a century and a quarter ago we find: "If 'tis wrote against anything, 'tis wrote an' please your worships against the spleen, in order by a more frequent and more convulsive elevation and depression of the diaphragm, and the succussations of the intercostal and abdominal muscles in laughter, to drive the gall and other bitter juices from the gall-bladder, liver and sweetbread of his Majesty's subjects, with all the inimicitious passions which belong to them, down into their duodenums."

It must, however, be recognised that many subjects which are most interesting to the physiologist either involve so much special knowledge, or are so beset with technical terms, that it is difficult to make clear to others even their general drift.

I am not without uneasiness that my subject to-day may be found to fall within this category. I propose to consider some relations of the nerves which pass from the brain and spinal cord, and convey impulses to the other tissues of the body—the motor or efferent nerves; and in especial the relations of those efferent nerves which run to the tissues over which we have little or no voluntary control. It is as well to say at once that none of the general conclusions which I lay before you are encrusted with universal acceptance. One or two have been subjects of controversy for the last fifty years; others are too young to have met even with contradiction. I do not propose to give you an account of the various theories which have been put forward on the questions I touch upon, nor do I propose to point out how far the views I advocate are due to others. I am concerned only to state what seems to me to be the most probable view with regard to certain problems which have been emerging from obscurity in recent year.

Limitations in the Control of the Nervous System over the Tissues of the Body.—In view of the conspicuous manner in which nervous impulses affect every-day life, we are perhaps apt to over-estimate the character and range of the influence exercised directly by the nervous system.

From the early part of this century one way of regarding the body has been to consider it as made up of tissues grouped together in varying number and amount. Each tissue has its characteristic features under the microscope. We need not enter into the question as to which of the commonly recognised tissues of the body are to be regarded as forming a class by themselves and which are to be regarded as subdivisions of a class. The point I wish to lay stress on is that in any broad classification not more than two tissues are known to be supplied with approximate completeness with efferent nerve-fibres. The striated muscular tissue, which forms, amongst other parts of the body, the muscles of the limbs and trunk, receives in all regions nerve-fibres from the brain or spinal cord. And the unstriated muscular tissue, which forms, amongst other parts of the body, the contractile part of the alimentary canal and of the blood-vessels, is in nearly, and possibly in all, regions similarly supplied.

The glandular division of epithelial tissue in some parts responds promptly and strikingly to nervous impulses, but in some parts the response is feeble, and in others no nervous impulse has been shown to reach the tissue. The connective tissue which exists all over the body, and which in its varied forms of connective tissue proper—cartilage, bone, teeth, epithelioid cells—makes so considerable part of it, is in mammals, so far as we know, destitute of efferent nerve-fibres. The epidermic cells, which form a covering for the body, the ciliated cells, the reproductive cells, do not visibly respond to any nerve stimulus. And the myriads of blood corpuscles, which in different ways are in incessant action for the general welfare, are naturally out of range of nervous impulses. According to our present state of knowledge, large portions of the organism live their own lives uninfluenced, except indirectly, by the storms and stresses of the central nervous system. No nervous impulse can pass to them to make them contract or to make them secrete, or to quicken or slacken their inherent activity. The nervous system can only influence them through the medium of some other tissue by changing the quantity or quality of the surrounding fluid.

Regarding, then, the body from the point of view of the control exercised by the nervous system on the other constituents, we have first to recognise that this control is in considerable part indirect only, that the several tissues are in varying degree under direct control, and that different parts of

one tissue may be influenced by the nervous system to different extents.

Limitation in the Control of the Nervous System over the different Activities of the Cell.—Even when nervous impulses can strikingly affect the vital activity of a tissue, their action is limited. They cannot modify the activity in all the various ways in which it is modified by the inherent nature of the tissue and the character of the surrounding fluid. Thus the sub-maxillary gland which pours saliva into the mouth is in life ceaselessly taking in oxygen and giving out carbonic acid; it does this without pouring forth any secretion. So far as we know, no nervous impulse can hasten or retard this customary life of the gland by a direct action upon it without producing other changes. The nervous system can only do this indirectly by modifying the blood supply. The nervous impulse which reaches the gland cells causes them to secrete, to take up fluid on one side and to pour it out on the other, and it does not, and so far as we know it cannot, confine its influence to those changes ordinarily going on in the gland cells. The essential effect of a nerve impulse appears to be to modify the amount of energy set free as work; usually it causes work to be done, as in the contraction of a muscle, or in the secretion of fluid by a gland; sometimes it diminishes the work done, as in the cessation of a heart-beat, or the decrease of contraction of a blood-vessel. Other changes often go on side by side with this setting free of energy as work, but there is no unimpeachable instance in which these other changes take place by themselves as the result of nervous excitation. Physiologists have sought for long years in all parts of the body for nerves—calorific or frigorific nerves—which cause simply an increase or decrease of the heat set free by a tissue; and for nerves—trophic nerves—which cause simply chemical changes in the tissue associated with a setting free of heat or not. Probable as the existence of such nerves seems to be, the search for them cannot, I think, be said to have been successful.

Somatic or Voluntary Tissues.—When we look at the question of nervous control subjectively, and consider in ourselves what tissues are at our beck and call, we find that we have immediate and prompt governance over one tissue only, the one which, as we have already seen, is most universally supplied with efferent nerve-fibres—namely, the (fibrous) striated muscular tissue. The parts of the body composed of this muscular tissue we move, as we say, at will. We exercise a control over it that we cannot exercise over any other tissue. The tissue is supplied with a special system of nerves. In other vertebrates there is a tissue of similar microscopical characters, and having a similar system of nerves. And we can be certain that in all vertebrates the fibrous striated muscle and the nervous system belonging to it form a definite portion of the body which can be properly placed in a class apart from the other tissues of the body. The tissues in this class are spoken of as "somatic" tissues, or sometimes, in view of our own sensations, as "voluntary." "Voluntary" is not a word which physiologists care much to use in this context, because it readily gives rise to misconceptions. It will serve, however, if we bear in mind that the primary distinguishing characters of the system are microscopical, anatomical and developmental; that other tissues than "voluntary" can be put in action by the will, though in a different fashion; and that "voluntary" tissues are also put in action involuntarily. That is to say, the word will serve if we rob it of much of its ordinary meaning.

The somatic or voluntary nervous system has in its essential features long been known. We may leave it and pass on to a more obscure field.

Autonomic or Involuntary Tissues.—In putting on one side the voluntary system, you will notice that we have disposed of one only of the several tissues, differing microscopically from one another, which go to make up the various organs of the body. Of the rest some, as we have said, either do not receive nerve-fibres from the brain and spinal cord, or, if they do, practically nothing is known about them in our own class of vertebrates—the mammalia. These I shall say a word or two about later. For the present we must confine our attention to the tissues which are known to be supplied not too illiberally with nerve-fibres. These are unstriated muscle, and its allied cardiac muscle, and certain glands. Since the voluntary striated muscle has a nervous system of its own, it might be imagined that the unstriated tissue and the glandular tissue, differing as they do, would also have separate nervous systems. This, however, is not the case. The nervous supply of these two tissues have common features and belong to the same system. There is, unfortunately, no satisfactory term by which to designate it. On the whole the term "autonomic" seems to me best adapted for scientific use. But it is not of the first importance for our present purpose to insist upon a proper nomenclature, so that I think I shall not do much harm if I use the familiar "involuntary" for the unknown, or nearly unknown, "autonomic."

I need hardly point out how widespread are both the glandular and the unstriated muscular tissues. In man practically the whole surface of the skin is supplied with sweat-glands, lachrymal glands lie hid behind the eye, small glands are thick in the respiratory tract from the nose to the smaller bronchial tubes, and glands stretch along the whole of the digestive tract. Most of these can be set in action by nerve-fibres. There are a number of others in which such action has not been shown, so that they do not concern us at present. Unstriated muscle forming, as it does, part of the walls of the arteries and veins, penetrates to every part of the body. It forms a large part of the coats of the stomach and intestines; it is present in the spleen and in parts of the lymphatic vessels; it is present in the iris and in other parts of the eye; it occurs in greater or less amount in different animals in the deeper layers of the skin.

Consider some of the ways in which these tissues in the several organs or structures affect the working of the body. The heart contracts and supplies the driving force for the circulation of the blood; the arteries contract less or more, here or there, and regulate the amount of the blood to each region; the digestive tract secretes solvent and disintegrating fluids in the food, churns it into pulp, absorbs some and rejects the rest; the skin-glands pour out their tiny beads of perspiration, and so aid in regulating the temperature of the body; the iris commands the aperture of the pupil and determines the amount of light falling on the retina; the ciliary muscle, by its varying contraction, brings about the focussing necessary for distinct vision.

But the involuntary tissues do not confine themselves to actions of such flagrant utility as those just mentioned. The contraction of small bundles of unstriated muscles in the skin will cause the flesh to creep; other similar small muscles are attached to the hairs; 'tis these will make

> "Thy knotted and combinèd locks to part,
> And each particular hair to stand on end,
> Like quills upon the fretful porpentine."

The involuntary tissues, although not under the prompt and immediate control of the will, are under the control of the higher centres of the brain. They are particularly responsive to the emotions; and in so far as we can call up emotions, we can play upon them at will. The ease with which nervous impulses pass along given tracts depends, amongst other things, upon use. And so it appears that our great-grandfathers wept and our great-grandmothers fainted with an ease which we should require assiduous practice to attain.

Further, you may note that the contraction of involuntary muscle caused by an emotion may in its turn set up nervous impulses, which pass back to the brain and give rise to vague and curious feelings, feelings often lending themselves to effective literary expression:—

> "Where our heart does but relent, his melts; where our eye pities, his bowells *yearn*."

I must ask your forgiveness for mentioning so many well-known facts in the sketch which I have just given of the involuntary tissues. But I hope it will take from you all excuse for not understanding the rest of what I have to say.

The arrangement of the involuntary nervous system presents some peculiar characters. The most distinctive of these is that the nerves, after they leave the brain or spinal cord, do not run interruptedly to the periphery; they end in nerve-cells, and the nerve-cells send off the fibres which run to the periphery. The most direct proof of this lies in the fact that a certain amount of nicotine prevents the central nervous system from having any influence on the peripheral structures—*i.e.* the line is somewhere blocked; it can be shown, speaking generally, that there is no block on either side of the ganglia, so that it must be in them. The actual point of attack of the nicotine appears to be the connections made by the central nerve-fibres with the peripheral nerve-cells. Thus all nerve-impulses, which pass from the brain or spinal cord to unstriated muscle or glandular tissue, pass through an intermediate station on their way. In

this, as in some other respects, the arrangement of the involuntary nervous system is more complex than that of the voluntary nervous system; in the latter the motor nerve-fibres run direct to the tissue and have no nerve-cells on their course. The nerve-cells which form the intermediate stations for the involuntary nerve-fibres are grouped together into ganglia; and so we may call the nerve-fibres which run from the brain or spinal cord to the nerve-cells pre-ganglionic fibres, and the nerve-fibres which run from the ganglia to the peripheral tissues post-ganglionic nerve-fibres.

The involuntary nervous system is divided into at least two subdivisions. The most extensive of these is what is called the *sympathetic nervous system*. The pre-ganglionic fibres of the sympathetic arise from a limited portion of the spinal cord. They arise from that part of the spinal cord which is in the region of the chest and the small of the back—*i.e.* roughly from the part which lies between the origin of the voluntary nerves for the arms and the origin of the voluntary nerves for the legs. The fibres given off by the ganglia of this system—*i.e.* the post-ganglionic fibres—run to the involuntary tissue in all parts of the body.

The Cranial and Sacral Systems.—The second division of the involuntary nervous system consists of two parts: one part—the cranial—arises from the brain—*i.e.* above the origin of the sympathetic; the other—the sacral—arises from the end of the spinal cord—*i.e.* below the origin of the sympathetic.

Each supplies a limited and different part of the involuntary tissue of the body, but both together supply a portion only of it. Taking the distribution broadly, they supply the muscular coats of the alimentary canal and certain structures connected developmentally with the anterior and posterior portions of it. They are especially connected with these terminal portions; they send numerous nerve-fibres to them; whereas they send but few to the intervening portion, and none at all to its blood-vessels. Thus parts of the involuntary tissue of the body receive a double supply of nerve-fibres, whilst parts receive a single supply only. Amongst the latter are all the involuntary tissues of the skin, the blood-vessels of the limbs and trunk, and of most of the viscera.

The cranial and sacral divisions of the involuntary nervous system are considered by some observers to be simply portions of the sympathetic system separated from it by the development of the nerve-centres for the arms and for the legs. I may give one reason why I do not take this view. The middle portion of the spinal cord, which is the region that sends fibres to the sympathetic, always sends fibres to a given spot by more than one nerve, and usually by four or five. The fibres passing by the several spinal nerves never differ in the kind of effect they produce, but only in the degree of effect; the difference is in quantity and never in quality. If, then, regions above and below were mere separated parts of this sympathetic region, we should expect that when one of these regions and the sympathetic region sent nerves to the same spot, the effect produced by both sets of nerves would be the same in kind, though it might differ in extent. But this is often not the case. Thus certain blood-vessels may receive nerve-fibres from four spinal nerves in the sympathetic region and from three spinal nerves in the sacral region; all the former cause contraction of the blood-vessels, all the latter cause dilation. And thus it seems to me probable that in the evolution of mammals the sympathetic nerves have developed at one time, and the cranial and sacral involuntary nerves have developed at another time.

Inhibition.—A striking feature of the involuntary nervous system is its possession of nerve-fibres which, when excited, stop some action at the time going on. The most striking example is perhaps the cessation of the heart-beats brought about by excitation of the vagus nerve. Such nerve-fibres are called inhibitory nerve-fibres, and the stopping of the action is called inhibition.

So far as has been definitely proved inhibitory nerve-fibres only run to involuntary muscle and to nerve-cells, and to these, so far as has been certainly shown, only in particular cases. It is true that when fear or other emotion causes the tongue to cleave to the roof of the mouth, there is a cessation of the customary flow from certain glands, but this flow is itself the result of nervous impulses passing in ever rising and falling intensity from the central nerve-cells, and its cessation is due to inhibition of nerve-cells, and not to inhibition of glandular cells.

The inhibition of nerve-cells has only been proved to take place in the central nervous system. When a group of nerve-cells of the central nervous system is engaged in sending out nervous impulses, other nervous impulses reaching them by way of other nerve-cells may diminish or stop their activity. The theory which is commonly advocated now to explain this inhibition makes the activity of the nerve-cells depend upon their receiving stimuli from the minute endings of other nerve-cells, and the cessation of the activity to depend upon these minute endings, either withdrawing themselves out of range, or having something interposed between them and the nerve-cells, so that the impulses can no longer pass. This theory I do not wish to discuss to-day; it is sufficient to say that if it is true, the inhibition of nerve-cells is an entirely different process from that of the inhibition of involuntary muscle.

Turning to the inhibition of involuntary muscle, there is a source of confusion which we must first guard against. Nearly all the unstriated muscle in the body is kept in a state of greater or less tone, or contraction, by the central nervous system. A diminution or cessation of this contraction may then be caused by a diminution or cessation of the activity of the central nervous system. This cessation of contraction is, of course, not what we mean by an inhibition of the unstriated muscle. It is usually spoken of as an inhibition of the nervous centre. The inhibition we mean is that which is caused by stimulating the peripheral end of a nerve outside the spinal cord.

I have said that this inhibition can only be obtained in certain cases, and it is not easy to find anything in common with regard to these cases. But on the whole it appears that the more a tissue is able to work by itself, the more likely it is to be under the control of inhibitory fibres. The heart, stomach and the intestines work when no longer connected with the central nervous system, and these are especially liable to inhibition.

There has been a marked tendency amongst physiologists, in considering the question of inhibitory nerve-fibres, to take what may be called *the view of the equal endowment of the tissues*. Because some arteries have inhibitory nerve-fibres, therefore it is to be held as in the highest degree probable that all have. And many would go further and say that it is therefore in the highest degree probable that all unstriated muscle, and glands, and even the voluntary muscles, have such fibres. This view seems to me a mistaken one. There is hardly room for doubt that the motor fibres are supplied in most unequal measure to the unstriated muscle and glands of the body. There are veins in the body containing unstriated muscle, which show no visible contraction from any nerve stimulation. And there are a number of glands which no nerve—so far as we know—excites to secretion. Since in the course of the evolution of the organism, a universal development of motor fibres has not occurred, it is, I think, to be expected that the development of inhibitory fibres should be still less universal. For up to a certain point the results of inhibition can be obtained in most cases without inhibitory nerve-fibres, by a simple diminution in the impulses travelling down the motor fibres. The only, and the final, test is of course experiment. But not all experiments are decisive, and theory inevitably colours interpretation. This theory of the equal endowment of the tissues has, it seems to me, caused a number of quite inconclusive experiments to be accepted as offering satisfactory evidence for the existence of inhibitory nerve-fibres.

Passing from this question, we may consider briefly how far we can get on the way to understand what occurs during inhibition. The external characteristic feature of inhibition is that a certain state of activity ceases; a muscle contracting at short intervals ceases to contract, or a muscle in a steady state of contraction loses this state. The tissue in either case becomes flabby.

The activity of a tissue may obviously be due to its receiving some stimuli from the nervous system or to its own inherent qualities. In the former case, if the tissue were only active when receiving nervous impulses, we should naturally look to some interference with these impulses as being the cause of inhibition. The blood-vessels of the sub-maxillary gland appear to me to offer sufficiently clear evidence with regard to the inhibition of blood-vessels. The superior cervical ganglion is the local centre from which the nerve-fibres bringing about contraction run to the blood-vessels of the gland. When this ganglion has been removed and the nerve-fibres from it have degenerated, the vessels receive no nervous impulses causing them to contract. But stimulation of the inhibitory nerve will still cause dilation—*i.e.* inhibition of the blood-vessels. The

inhibition must then be due to a direct action on the tissue, and not to an interference with other nerve impulses. The evidence with regard to the inhibition of the beat of the heart and of the tone or peristalsis of the alimentary canal is more complex, but there is good reason to believe that the contraction is in both cases due to their inherent qualities. And if this be granted, it follows that here also inhibition must be due to a direct action upon the tissue.

The contraction of a muscle is due to a chemical change in it. In this chemical change some energy is set free as work—shown by the contraction of the muscle—and some as heat. It is conceivable that the nervous stimulus which causes inhibition should cause all the energy set free by the chemical change to take the form of heat. In that case the inhibitory nerve would be a calorific nerve. The amount of chemical change is indicated by the amount of carbonic acid given off to the blood. No experiments have been made as to the amount of carbonic acid given off to the blood by an inhibited tissue, but it appears very unlikely that the amount is increased, and we may take this view of the action of an inhibitory nerve as improbable.

If the nervous impulse does not act in this way it must in some way stop the particular chemical change associated with contraction from taking place. It does not stop all chemical change, for blood passing through an inhibited tissue loses some of its oxygen. The simplest way for a nervous impulse to prevent a particular chemical change is to induce a different one. We have seen that the tissues which are inhibited have a great tendency to contract of themselves—that is, they form certain very unstable substances. In closely related tissues which are not inhibited this tendency exists but little or not at all. The

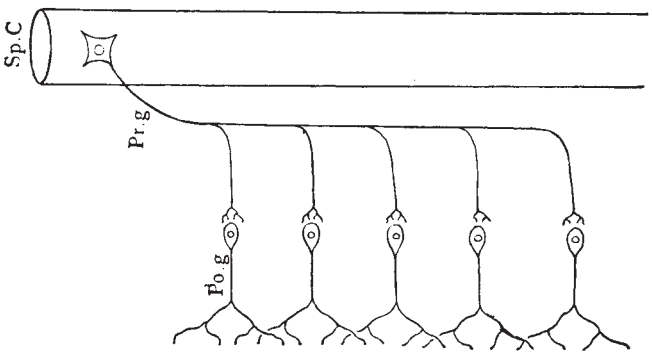

FIG. 1.

proximate cause of inhibition might then be that the nervous stimulus causes certain molecules of the tissue to form more stable combinations. This need not be associated with any general assimilation; it would simply make the muscle adopt for a time a mode of life more like that of other closely related muscle.

Number of Relay Stations.—I have already mentioned that the nerve-fibres which pass from the central nervous system to the involuntary tissues do not run to it direct, but end in groups of nerve-cells or ganglia from which fresh nerve-fibres are given off. Now, in most cases, there are anatomically several ganglia on a nerve in its course from the spinal cord to the periphery. For example, the nerve-fibres which cause the hairs of a cat's tail to stand on end, giving the tail the appearance of a bottle brush, leave the spinal cord in the lower part of the back, and enter a nerve-strand which is beaded with ganglia. They leave this strand near the root of the tail. Between the point where the nerve-fibres enter and the point where they leave the strand there are seven or eight ganglia. The fact offers us a problem of some difficulty. With how many of these ganglia are the nerve-fibres connected? Or, in other words, how many relay stations are there—eight or one, or some intermediate number? Further, do all kinds of involuntary nerve-fibres in all parts of the body have the same number of relay stations, or do some have one, some two, some three, and so on? It would take too long to discuss this question here. But the experimental evidence is, I think, fairly decisive in favour of the simple view that the nerve-impulse passes through one relay station only. There is, however, evidence that the nerve-fibres which pass from the spinal cord branch, so that we may take the element by reduplication of which the involuntary nervous system is built up to be diagrammatically as in Fig. 1.

Reflexes.—Another point of view is given by a comparison of the groups of nerve-cells of the peripheral ganglia with the groups of nerve-cells of the brain and spinal cord. The proper working of the body depends upon an agile response by the central nervous system to what is going on in the periphery. Now the peripheral ganglia are made up of nerve-cells and

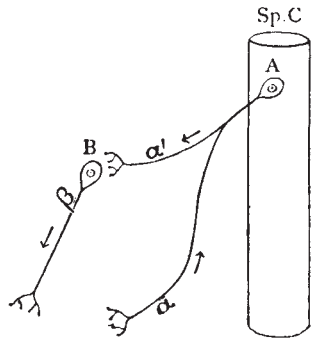

FIG. 2.

nerve-fibres which differ less in general characters from some of the cells of the central nervous system than these differ from one another. The nerve-cells of the spinal cord can receive impulses from many groups of nerve-cells both near and remote; they do not simply receive impulses from one quarter alone—say, the cortex of the cerebral hemispheres—but from many quarters, and notably direct from the periphery. Hence it has been supposed that the peripheral ganglia have similar wide connections, that they receive impulses direct from the periphery, that each is connected with other ganglia, and that impulses received from the periphery, or elsewhere, bring separate ganglia into coordinate action. And this view, which has been taken on general grounds, has been supported by microscopical observations.

The evidence against this view is of two kinds. In the first place, it can be shown that in a number of individual cases the nerve-cells of one ganglion have no connection with the nerve-cells of another ganglion, so that anything like a universal scheme of connection is out of the question. And, secondly, it can be shown that whenever an action occurs, which might be referred to such connection, it is an action which is bound to occur in consequence of some other known arrangement, and that therefore it is unnecessary to seek for a further cause.

The evidence of the first kind we need not enter into; the evidence of the second kind we may hastily touch on. If we accept the conclusion stated above, that the pre-ganglionic nerve-fibres branch, and the branches run to different nerve-cells, it follows that a stimulus applied to one branch will stimulate a

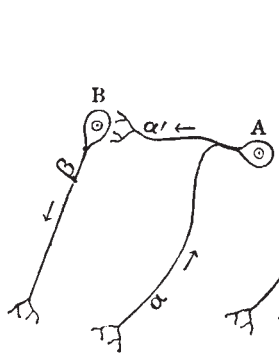

FIG. 3.

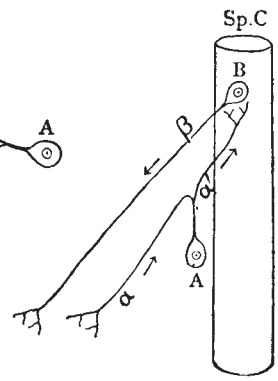

FIG. 4.

number of nerve-cells; this follows since a nerve-impulse set up in any part of a nerve travels over the whole of it. Thus actions, resembling reflex actions, will inevitably be obtained whenever nerve-fibres are stimulated which send branches to different ganglia. The mechanism in this case is confined to motor nerve-fibres and nerve-cells. The action, for lack of a con-

venient term, was spoken of by Dr. Anderson and myself as a reflex action. It is perhaps better to call it a *pseudo-reflex action.*

Regarded from the customary point of view, a pseudo-reflex differs widely from a reflex action. The one is brought about by stimulating an efferent or motor fibre, and the other by stimulating an afferent or sensory fibre.

But suppose we compare them from another point of view. Fig. 2 is a diagrammatic representation of a pseudo-reflex. A nervous impulse passes up one branch α of a cell A, passes to another branch α', so excites a cell B and its nerve-fibre β.

Fig. 4 is a diagrammatic representation of a simple true reflex in the voluntary muscle. A nervous impulse passes up one branch α of a cell A, passes to another branch α', so excites a cell B and its nerve-fibre β.

You see the two can be described in exactly the same terms, and both are reducible to the diagram of Fig 3. It is true that the cells A and B are not similarly situated in the two cases; in the pseudo-reflex A is in the spinal cord, and B is outside it in a peripheral ganglion; whereas in the true reflex A is outside the spinal cord, in a spinal ganglion, and B is inside the cord. But then no one has even suggested that the position of a nerve-cell determines whether an action in which it takes part is a reflex or no. So that this point is irrelevant. And so it might be urged that the one action has as good a title to be called a reflex as the other. I do not, however, wish to insist too much on this comparison. I am inclined to say, after Touchstone, "An ill-favoured thing, sir, but mine own."

If, as some think is the case, the spinal ganglion cell receives the nerve-impulse conveyed by the peripheral nerve process, and modifies it before passing it on to the central process, this establishes a distinguishing character for the true reflex. It would be probably an axon plus dendron reflex, the pseudo-reflex being simply an axon reflex. The important known functional difference between the reflex and the pseudo-reflex is that in the former case the nerve-endings of the primarily affected nerve-fibre are specially differentiated for receiving nerve-impulses, and in the latter case these endings are specially differentiated for imparting nerve-impulses. And, on the whole, it is probable that the pseudo-reflex is not a normal part of the working of the body, but comes into play only as it were by accident. I do not, however, regard this as quite certain.

The pseudo-reflex I have spoken of is caused by the excitation of nerve-fibres before they reach the ganglia—*i.e.* of pre-ganglionic fibres. But the fibres which are given off by the ganglia also branch, so that it appears inevitable that we should have in certain circumstances an action related to a reflex caused by a stimulation set up in one of these branches spreading to the rest—*i.e.* a spreading out of impulses in post-ganglionic fibres similar to that which occurs in pre-ganglionic fibres. Turning to the diagram, Fig. 1, a nervous impulse set up in one branch—possibly by a contraction of muscle-cells to which it runs—would spread to other branches and cause contraction of the muscle-cells in connection with them. You will notice that this spreading out of impulses does not necessarily involve the stimulation of any nerve-cell; it might perhaps be distinguished as *irradiation.* It would, probably, be very local in action, unless there were overlapping of the districts supplied by the several nerve-cells, in which case a not inconsiderable spreading out of a local contraction might take place, giving rise to a peristaltic wave.

It must be pointed out that it has been assumed that in the sympathetic nervous system an impulse cannot pass from a motor fibre through the nerve-cell from which the fibre arises and affect any other nerve-fibre or nerve-cell. There is good ground for this assumption, but the experimental evidence might certainly be more complete.

To return to our main line of argument, we have good evidence that nervous impulses set up in one spot may affect regions more or less remote by a mechanism which does not involve the presence in the sympathetic system of special sensory nerve-cells with peripheral sensory nerve-endings. And so far as investigation has gone at present, I think that all the apparent reflex actions can be explained without reference to such sensory apparatus. And so I take the analogy of the peripheral ganglia with the central nervous system to be misleading, and consider that all the nerve-cells of which we have been speaking are motor nerve-cells, and that they all conform to the simple plan shown in Fig. 1. Thus the whole consists of a duplication of one type; a cell in the spinal cord which branches, each branch ending in a single cell; each of these cells sends off a nerve-fibre which branches, the branches ending in a group of involuntary muscle or gland cells.

That I regard as the real working mechanism, but there are two reservations to make. All the tissues of the body may be looked upon as engaged in a lifelong process of carrying out experiments, and I am prepared to believe that there are in the body what may be spoken of as the residues of these natural physiological experiments, either the beginnings of experiments which have not succeeded, or the melancholy ends of those which once partially successful have failed later. Such possibly may be the nerve-cells which have been described in sympathetic ganglia as sending their nerve-fibres to other nerve-cells.

Secondly, in this account I have not included the nerve-cells which exist in the wall of the alimentary canal, and the cells of Auerbach's and Meissner's plexuses. These "enteric" nerve-cells belong, I hold, to a system different from that of the other peripheral nerve-cells. With regard to their connections I do not think anything can be said with certainty.

Regeneration. Specific Nerve Energy.—One other problem presented by this involuntary system we may say a few words about. You know that when a nerve in the hand or arm is cut the nerve will in proper conditions grow again; and the lost feeling and the lost power over the muscles will return. The recovery is brought about by the part of the nerve which is attached to the spinal cord growing along its old track and spreading out as before in the muscle, skin and other tissue. At any rate, that is the method for which there is most evidence. You may know also that when the nerve-fibres in the spinal cord are similarly injured, they do not recover function. Regeneration in the latter case implies that the nerve-fibres have to form fresh endings in connection with nerve-cells. If this were more difficult than the formation of nerve-endings in muscle and other non-nervous tissues, the difference which exists as regards recovery of function between the nerve-fibres of the limb and nerve-fibres of the spinal cord would be readily explainable. But recent experiments show that the nerve-fibres which run from the spinal cord to the peripheral ganglia—*i.e.* pre-ganglionic fibres—re-form with ease their connection with nerve-cells, so that we may probably seek in mechanical conditions for the reason of the absence of regeneration of the fibres in the spinal cord. Possibly some way may be found of improving the mechanical conditions, and so obtaining regeneration. That question, however, we need not enter into.

The regeneration of the pre-ganglionic nerves presents some very remarkable features. The nerve-fibres which end in a sympathetic ganglion are rarely, if ever, all of one kind—that is to say, they do not all produce the same effects. Thus, of those which run to the ganglion in the upper part of the neck, some cause the eyelids to move apart, some cause the pupil to dilate, some cause the face to become pale, some cause the glands of the mouth or skin to secrete, and others have other effects. These different kinds of nerve-fibres run, in large part at any rate, to different nerve-cells in the ganglion. There are in the ganglion several thousands of nerve-cells closely packed together. And it would seem hopeless for each kind of nerve-fibre as it grows again into the ganglion during regeneration to find its proper kind of nerve-cell. Nevertheless, nearly all of them succeed in doing this. The nerve-fibres which normally cause separation of the eyelids, or dilatation of the pupil, or pallor of the face, or secretion from the glands, produce the same effects after several inches of their peripheral ends have formed anew.

The fact offers at first sight a striking proof of a specific difference between the different classes of nerve-fibres and different classes of nerve-cells. Through the matted mass formed by the delicate interlacing arms of the nerve-cells, the ingrowing fibres pursue their tortuous course, passing between and about hundreds of near relations until they find their immediate stock, whom they clasp with a spray of greeting tendrils and so come to rest.

Absolute laws seem unfitted for a workaday world. For closer observation shows that the fibres have not always this marked preference for their own stock. The nerve-fibres of the cervical sympathetic, the nerve I have spoken of above, do not often go astray, at any rate so far as is known. But they do sometimes; thus it may happen that some nerve-fibres which

ought to find their home with nerve-cells governing the blood-vessels, take up with nerve-cells governing the dilator structures of the pupil.

And if we turn to other nerves, greater aberrations are found. We have seen that the nerves running from the central nervous system to involuntary structures may be divided into two sets: the sympathetic nerves on the one hand, and the cranial and sacral nerves on the other. An important cranial nerve is the vagus; it causes, when in action, cessation of the heart-beat, contraction of the œsophagus, contraction or inhibition of the stomach, and various other effects. It does not send nerve-fibres to any of those structures of the head which we have seen the sympathetic ganglion at the top of the neck —the superior cervical ganglion—so liberally supplies. And yet the vagus nerve, if it has a proper opportunity of growing into the superior cervical ganglion, will do so, and there establish connections with the nerve-cells. Thus the nerve which properly exercises control over certain viscera in the thorax and abdomen is capable of exercising control over structures in the head, such as the iris, the blood-vessels and the glands. The details of the process, with which I will not trouble you, do not afford any clear evidence that the nerve-fibres of the vagus pick and choose amongst the nerve-cells of the superior cervical ganglion; the fibres appear rather to form their terminal branches around any kind of nerve-cell, so that, in fact, the action which the nerve-fibre will in future bring about depends, not on any intrinsic character of its own, but upon the nature of the action carried on by the nerve-cell. The nerve-cell may cause secretion from a gland, or contraction of a blood-vessel, or dilation of the pupil, or movement of hairs; whichever action it causes, the nerve-fibre which joins it from the vagus nerve can cause for the future, and it can cause no other. In this case, then, we arrive at results which are hopelessly at variance with the view that the nerve-fibres and nerve-cells of the involuntary nervous system are divided into classes which are fundamentally different. In other words, that theory which is spoken of as the theory of specific nerve-energy does not apply here.

But if this is so, how are we to account for the selective power shown by the sympathetic nerve-fibres which I have mentioned earlier? That the different classes of nerve-fibres and nerve-cells with which we are dealing have not those deep and inherent differences which are required by the theory of specific nerve-energy is, it seems to me, certain. Nevertheless, there may be some differences of a comparatively superficial nature which suffice to explain the selective activity observed. We may suppose that a re-growing nerve-fibre will in favourable circumstances join a nerve-cell the function of which is the same as that of its original cell, but that if there are hindrances in the way of this return to normal action, and if the conditions are favourable for joining a nerve-cell acting on some other tissue, why then it will join this. It is as if it had a preference, but did not care overmuch. We might perhaps express the facts by saying that there are different varieties of pre-ganglionic fibres, but no species.

We have been speaking so far of the nerve-fibres which run from the brain and spinal cord to the peripheral nerve-cells. The nerve-fibres which run from the peripheral nerve-cells have also, there is reason to believe, a large measure of indifference as to the kind of work they perform. The limits of this indifference have yet to be investigated.

I have said earlier that in mammalia nerve-fibres are not known to run to connective-tissue cells or to epidermic cells. But in some lower vertebrates certain connective-tissue cells are under the control of the central nervous system. Thus in the frog the pigmented connective-tissue cells, which play a large part in determining the colour of the skin, can be made to contract or to rearrange their pigment granules—and so change the colour of the skin—by excitation of certain nerves. In all probability, the motor nerve-fibres to the pigment-cells belong to the same class as the nerve-fibres which run to the arteries and to the glands—*i.e.* they belong to the autonomic system. We have seen that unstriated muscle-cells and gland-cells in different parts of the body are by no means equally supplied with motor nerve-fibres, and it may be that in mammals there are certain connective-tissue cells which receive motor nerve-fibres. Further, if it is true, as it well may be, that nerve-fibres which run to a gland are capable in favourable conditions of making connections with a blood-vessel, it is not beyond hope that either kind of nerve-fibre may experimentally, by offering it favourable conditions, be induced to join connective-tissue cells.

The factors which determine whether a particular tissue or part of a tissue is eventually supplied with nerve-endings, and the degree of development of these, are the factors which determine evolution in general. In the individual, it is exercise of function which leads to the development of particular parts; in the race, it is the utility of this development which leads to their preservation. And so it is conceivable that in some lower vertebrate at some time, the autonomic nervous system may have developed especially in connection with those tissues which appear in ourselves to be wholly unprovided with motor nerve-fibres.

I am tempted, before ending, to make a slight digression. Those who have occasion to enter into the depths of what is oddly, if generously, called the literature of a scientific subject, alone know the difficulty of emerging with an unsoured disposition. The multitudinous facts presented by each corner of nature form in large part the scientific man's burden to-day, and restrict him more and more, willy-nilly, to a narrower and narrower specialism. But that is not the whole of his burden. Much that he is forced to read consists of records of defective experiments, confused statement of results, wearisome description of detail, and unnecessarily protracted discussion of unnecessary hypotheses. The publication of such matter is a serious injury to the man of science; it absorbs the scanty funds of his libraries, and steals away his poor hours of leisure.

Here I bring my remarks to a close. I have endeavoured to give as clearly as possible what seem to me to be the conclusions which logically follow from certain data, but I would not have you believe that I regard them as representing more than the immediate point of view. As the wise man said: "Hardly do we guess aright at things that are upon earth, and with labour do we find the things that are before us."

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

THE presidential addresses delivered before the Sections of Zoology and Botany of the American Association, by Profs. S. H. Gage and C. R. Barnes, respectively, are printed in *Science* of September 8. The subjects were "The Importance and the Promise in the Study of Domestic Animals" and "The Progress and Problems of Plant Physiology" and the subjoined extracts show some of the points dealt with. Abstracts of several other sectional addresses have already appeared in NATURE, p. 515.

Prof. Gage described a few ways in which the study of domestic animals has thrown light on the problems confronting mankind in his social ideals, in preventive medicine, in physiology and hygiene, in embryology and comparative anatomy and in the doctrine of the evolution of organic forms. He showed that, with the higher forms at least, that is the forms most closely related to man, and with whose destiny his own economic, hygienic and social relations are most closely interwoven, the domestic animals have in the past and promise in the future to serve the best purpose because of the abundance of the material in quite widely separated groups of animals which long have been and still are under greatly differing conditions and surroundings; and, finally, because this material is plentiful and under control, and thus may be studied throughout the entire life cycle.

There has been and still is too great a tendency in biology to study forms remote and inaccessible. This is, perhaps, partly due to the fascination of the unknown and the distant, and the natural depreciation of what is at hand. But study of these supposedly generalised types has proved more or less disappointing. No forms now living are truly primitive and generalised throughout. They may be in parts, but in parts only. The stress of countless ages has compelled them to adjust themselves to their changing environment, to specialise in some directions so far that the clue through them to the truly primitive type is very much tangled or often wholly lost. Indeed, every group is in some features primitive.

As any complete study requires much material at all stages the higher forms must be of the domesticated groups, or wild